LETTERS TO THE EDITOR.

**** The attention of scientific men is called to the advantages of the correspondence columns of* SCIENCE *for placing promptly on record brief preliminary notices of their investigations. Twenty copies of the number containing his communication will be furnished free to any correspondent on request.*

The editor will be glad to publish any queries consonant with the character of the journal.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

Wind Pressure and Velocity.

THE importance of an accurate determination of the relation between the pressure and velocity of the wind will be readily recognized. This relation is especially needed by the architect and bridge-builder, since most instrumental determinations are of the wind's velocity. The problem is much more intricate than is ordinarily supposed, and the diverse results obtained by experimenters of great ability show how the determination of the movements and behavior of gaseous media are hedged about with difficulty, and, as already pointed out in *Science* for July 8, the absolute necessity of building up the science of meteorology on a firm foundation of fact rather than theory.

There have been two methods of experimentation: the earliest, with plates rotated upon an arm seldom exceeding 10 feet in length, and, later, by the exposure of plates to direct air-motion. Borda, in 1763, with plates ranging from 16 to 85 square inches area, obtained the following relation,

$$p = (.0031 + .00035\,c)\ S v^2,$$

in which $p =$ pressure in pounds on the plate, $c =$ contour of plate in feet, $S =$ surface in square feet, and $v =$ velocity in miles per hour (this notation will be maintained throughout).

In 1874, Hagen tried most careful experiments with an arm of 8 feet. The velocity ranged from 1 to 3 miles per hour, but the room was so small that at the latter velocity the air was set in feeble rotation. The plates ranged in size from 4 to 40 square inches. He found, as did Borda, that the pressure per square foot increased with the size of the plate. The following is the relation established by him,

$$p = (.002894 + .0001403\,c)\ S v^2.$$

This formula for this relation has repeatedly appeared in print, and each time it has been changed. This is believed to be correct.

Singular as it may seem, these experiments have been almost the only ones quoted in discussions of this question, and yet it is easy to see that they are utterly useless for determining the pressure of a 50-mile wind on the side of a building.

In November, 1886, a few experiments in Washington with an arm of 4 feet, and plates from 16 to 576 square inches area, gave the relation,

$$p = (.0032 + .00034\,c)\ S v^2.$$

The agreement with Borda's results is very interesting.

Afterward, with the same style of apparatus and an arm of 16 feet, the relation found was

$$p = .0034\ S v^2.$$

The velocity of the larger plates was only 4 miles per hour, so that this formula does not help us for greater velocities. It was certainly established that there was no difference in pressure per square foot depending on the size of the plate. Turning to experiments of the second class, we find that Thibault obtains, with plates from 1 to 1.5 square feet area exposed to the wind, the relation,

$$p = .00475\ S v^2.$$

In France, with plates exposed on a locomotive running 44 miles per hour, the relation established was

$$p = .00535\ S v^2.$$

In this case, probably, a slight allowance must be made for the wind with the train.

In the 'Encyclopædia Britannica,' article 'Hydromechanics,' the mean of all the better determinations is

$$p = .00496\ S v^2.$$

We may conclude, 1st, that experiments with whirling arms of less than 16 feet are very untrustworthy; 2d, that we need determinations with rapid, straight-line motion, best obtained, perhaps, by pushing two or three platform-cars loaded with iron in front of a locomotive, exposing the plates on the front car; 3d, the relation $p = .005\ S v^2$ is the most satisfactory yet determined, and does not differ by more than four or five per cent from the truth.

While there has been this great difficulty in determining the above relation, there has been just as much, if not more, in connection with the relation between the velocity of the wind and that of the cups of Robinson's anemometer. Some confusion has arisen from the fact that the standard anemometer in England has 9-inch cups and 24-inch arms, while in our country we have 4-inch cups and 7-inch arms.

It has been determined, by careful experiment in England, that, if the large type of anemometer has a factor of 2.5, then the smaller should certainly have 3.00. Dr. Robinson, after a long research with a whirling machine, decided that the factor (of the smaller instrument probably) should be about 2.5. After trying a few experiments in the open air, however, he changed his view, and decided that the factor should be 3.00. In Washington, with an arm of 16 feet, and a velocity of 12 miles per hour, the factor was found to be 3.00.

Quite recently the Chief Signal Officer, through the kindness of the officials, as a preliminary to carrying on experiments on platform-cars, as suggested above, has had an anemometer placed upon a locomotive of the Baltimore & Ohio Railroad running from this city to Baltimore. Only one round trip has been tried thus far: in the outward trip the velocity of the train was about 20, and returning it was about 46 miles per hour. Allowing for the actual wind, we find the anemometer indication 46 miles going, and 47 returning. The distance was 40 miles, and we may consider that the excess of about 6 miles was due to the heaping up and flowing over of the air in front of the locomotive. All things considered, it seems probable that the factor 3.00 now used in our anemometers of 4-inch cups and 7-inch arms is entirely correct: certainly no change in the present factor can be thought of for an instant. A complete discussion of this question has already been prepared by me, and will appear in October. The other side of this question has been recently presented by Professor Ferrel in the August *American Meteorological Journal.* H. ALLEN HAZEN.

Washington, Aug. 22.

The Formation and Dissipation of Sea-Water Ice.

MR. W. A. ASHE'S opinion on the freezing-point of sea-water, and the conclusions he draws from his experiments, cannot be accepted. The arrangement of the experiment described in No. 228 of *Science* seems to be insufficient. A hole was cut through ice 87 centimetres (2.85 feet) thick. The water within was thoroughly agitated by stirring from below, and during the actual observation slightly agitated. The thermometer was held nearly horizontally, the bulb slighty lower than the rest of the instrument, just below the surface of the water. When the ice-film began to form, the reading of the thermometer was — 2°.9 C. (26°.7 F.), the temperature of the air being — 24°.8 C. (— 12°.6 F.). The greatness of the difference between the freezing-point of the sea-water and the temperature of the air detracts from the value of these observations. The ice is forming so rapidly that brine is included among the crystals: it is even probable that cryohydrates are formed at the surface. On the other hand, the freezing-point of sea-water was not only found by melting sea-water ice, as Ashe assumes, but also by freezing sea-water, and was always found to be between — 1°.6 C. and — 1°.8 C. (29°.1 and 28°.8 F.), according to the concentration of the solution. Mr. Ashe's second remark on this subject in No. 232 of *Science* does not agree with Buchanan's interesting researches on the melting of fresh-water ice in solutions of salts. He has shown by an excellent series of experiments (*Nature*, April 28 and May 5, 1887), that, when sea-water is frozen to the extent of fifteen per cent of its mass, and the crystals so formed are allowed to melt in the liquid in which they have been produced, they melt exactly as they have been formed. If snow or pure ice be immersed in the brine formed by partially freezing sea-water, it melts at the same temperature as the ice which had been formed by freezing the sea-water, so long as the chemical composition is the same in each case.

In a third letter to *Science* (No. 237), Mr. Ashe makes some remarks on the formation and character of Arctic ice. He says, that, as the density of sea-water increases till the freezing-point is reached, ice is not formed at the surface, but at a certain depth. In fact, the

cooling goes on from the surface, and is far more rapid than the motion of the water to deeper layers: therefore ice is formed before the cooled masses sink to the depth and are replaced by warmer ones.

I can confirm Mr. Ashe's observation that the new ice frequently forms first on projecting points. It seems to me that particles of ice which have formed on the shore are driven by wind and waves into the sea, and act as so many centres of congelation. As these particles and small cakes are drifting before the wind, long, narrow streamers are formed, each new opening being rapidly filled by new ice. These narrow strips do not consolidate, but are driven to a lee coast, or their motion is stopped in some other way. Then they are pressed together, the small cakes are broken and overflowed by ripples, the water freezes on top of them, and within a short time an extensive field is formed consisting of numerous small cakes pressed and piled upon one another, and cemented by the water that has overflown them. Besides this kind of ice, which is formed while the wind is blowing, smooth floes are formed in small bays during calm weather. The latter, however, form the smaller portion of Arctic ice. This process accounts for the great quantity of brine contained in the new ice. The deeper layers contain far less salt than the surface ice, as the mechanical admixture of water takes place on the surface only. The snow, on falling on this kind of ice, forms a slush, as it melts in the brine oozing out of the ice. This mixture freezes, and thus the thickness of the ice increases on the upper and lower sides simultaneously. The different origins of the layers of ice accounts for their different character.

During the winter the ice undergoes remarkable changes. The brine contained in ice of different depths is the more concentrated the lower the temperature of the layer. It is probable that the cavities between the ice-crystals are sufficient to effect a gradual decrease in the concentration of the upper parts, which then begin to freeze, and thus become drier.

When in spring the ice begins to melt, it loses its salt rapidly, and I observed in latitude 70° north, on the west coast of Baffin Bay, that it had become entirely fresh about the end of June, the brine being removed through the capillary cavities.

A remarkable effect of mechanical action upon the dissipation of the sea-water ice may be observed at places where rapid tides are running. When the temperature of the air rises to about —20° C. (—4° F.), the ice becomes saturated with water, and is being worn off at its lower side. It seems that the lower surface of the ice is rough, consisting of ice-needles with isolated points. These are broken off by the violent motion of the water, and thus by the friction of the broken pieces the volume of the ice-sheet is continually being diminished at its lower side. In winter the same process must be going on, though not to so great an extent. Some places of this kind are open throughout the winter. Their extent is changing according to the strength of the tides; and during the spring-tides ice of about fifteen or twenty centimetres thickness, which was formed during the preceding neap-tide, is broken up and pressed under the neighboring floe: consequently the thickness of this ice would continually increase if it was not worn off at its lower surface. In fact, the ice in such places is very treacherous and thin: therefore it seems, that, according to the strength of the current, a certain low temperature is required to resist its destructive influence. Below the layer which has this temperature the ice is saturated with water and being worn off.

Mr. Ashe touches upon another subject the explanation of which seems to me insufficient. On steep coasts in the Arctic regions an ice-wall is found attached to the rocks, and reaching from high-water to low-water mark, gradually decreasing in thickness, and having a vertical side. When the tide ebbs, the wet rocks are exposed to the cold air, and of course are covered with a thin sheet of ice. This process begins before the sea is frozen over, and, as the water is still agitated by winds, the thickness of the layer formed during a single tide is considerable. This process is going on throughout the winter, and thus this ice-foot continues to increase in width. At spring-tide its level is overflown by water, which adds to its height. It is characteristic of this ice-foot that the unbroken land-floe extends to its foot, a single crack separating the two masses. Cliffs which are washed by water throughout the winter have frequently no ice-foot at all. This shows that the floe favors its formation.

The ice on shores with a gradual slope is quite different. Here it is not attached to the bottom, as Mr. Ashe's description would imply, but originates in the following way. The ice which is formed on the surface at high water strands during the ebb-tide, and its volume is increased by the freezing of the water left on the beach. When the tide comes in again, the greater part of this ice begins to float, increases in thickness, and strands again on the rocks. By the repeated breaking of these masses on the rocks, the water is frequently exposed to the air, freezes, and thus the thickness of the ice is far more rapidly increasing than that of the unbroken floe. It is continually growing on all sides, new material being also added on the sides, and thus a heavy pressure results which affects the neighboring parts of the floe, which is frequently pressed under the level of the sea, is overflown, and thus increases in thickness. If the slope is sufficiently gentle, this part of the floe strikes also the rocks at low water, and is added to the growing belt of grounded broken ice that surrounds the coast. Similar forms of ice are found on mud-beaches, and I believe that such is the origin of the unusually heavy mud-colored masses of Fox Basin, which is known to be extremely shallow.

The formation of the ice-foot which was described above is similar in origin and appearance to the frozen freshets which in winter form ice-walls that are firmly attached to the steep cliffs. This proves that Mr. Ashe's explanation of this fact is not correct, and that fresh water as well as sea-water may freeze firmly to the ground.

DR. FRANZ BOAS.

New York, Aug. 19.

The Geologists' Congress.

PERMIT me to say a word concerning the generally fair and full report, in *Science*, of the proceedings in Section E of the American Association. By a typographical error, a clause in the conclusion of the digest of the reporter on the Archæan is made to read "American geologists will acquiesce in the recommendations of the *committee*," etc. 'Congress,' not 'committee,' was the word used. Again, "the recommendation that all pre-Cambrian rocks should be called Archæan savors too much of pre-judgment, especially in view of the recent studies of Irving and Walcott."

These studies, which have resulted in the theory called by Professor Walcott, in his letter to the reporter, 'Prof. R. D. Irving's view,' were very carefully considered in the body of the report. The sentence above gives no idea of the entire recommendation; of how far it differs from the view of Irving, Chamberlin, Walcott, and others; nor of why it seems to best reconcile the conflicting views expressed by American geologists. 1st. The term 'Archæan' as originated by Dana received the unanimous suffrages of the Berlin Congress, and, so far as the reporter could ascertain, has the indorsement of a very large majority of American geologists as a general term to cover all pre-Cambrian rocks. Irving would separate these rocks into two divisions of equal rank: to the lower (Laurentian) 'Archæan' should be applied, the upper (separated by a physical break from the first, and containing an unspecified number of smaller breaks or unconformities) he would erect into a new group of equal rank with Archæan in its new sense, and would call it, after Chamberlin, 'Agnotozoic.' He recognizes "great unconformity between the Cambrian and the Agnotozoic, besides which there are minor, though still quite extensive, unconformities between the members of the Agnotozoic itself." It is evident, from the tenor of all the views expressed on this proposed new division, that its exact rank is not certainly understood. All who recognize it believe that it has at least as high a rank as 'Paleozoic,' 'Mesozoic,' etc., but none can yet affirm that it may not consist of several such groups, divided by one or more of these 'extensive unconformities.' In view of this fact, and also of the circumstance that to a vast majority of geologists to-day the Archæan includes all pre-Cambrian rocks, it was thought that the recommendation of the reporter avoided, to the greatest degree, any pre-judgment of this question. It is as follows:—

"The division first in order of time shall have a rank of the first order, and shall be called 'Archæan.' (*a*) It shall comprehend all the rocks of origin anterior to the Cambrian. (*b*) The lowest sub-division of the Archæan shall be called the 'Laurentian.' (*c*) A division between the Laurentian and the Cambrian, provisionally